

Water Partitioning and Availability: Impact of Hydrology Shifts on Ecosystem Processes in a Bay–Delta Watershed

Rohit Salve

Public Comments

No public comments were received for this proposal.

Collaboration Panel Review

Proposal Title

#0056: Water Partitioning and Availability: Impact of Hydrology Shifts on Ecosystem Processes in a Bay–Delta Watershed

| Final Panel Rating |
|--------------------|
| inadequate |

Collaboration Panel (Primary) Review

Collaboration:

Will the results of the collaborative effort be greater than the sum of its parts? Is it clear why the subprojects are part of a larger collaborative proposal rather than several independent smaller ones?

inadequate

the EBMUD has only a small part of the study--mainly assisting with field observations. it appears that they will conduct ecological studies ater this study is done.

Interdependence And Integration:

Does the proposal have an example that clearly articulates the conceptual model of each subproject and how they link together as a whole? Are the boundaries of the study plans focused and cohesive, yet well delineated? Is there a plan for potential differences in the stages of subproject completion times? Are there clear plans for analyses and interpretations which seek to identify and quantify relationships among the data collected in various subprojects rather than separate analyses for each subproject?

adequate

all aspects of a comprehensive integrated hydrologic study of a small watershed is planned

Project Management:

Is it clear who will be performing management tasks and administration of the project? Are

#0056: Water Partitioning and Availability: Impact of Hydrology Shifts on Eco...

Collaboration Panel Review

there resources set aside for project management and time given for investigators to collaborate? Is there a process for making decisions during the course of the project? Are there acknowledgments of potential barriers to collaboration and explanations of how team members will overcome barriers particular to their institutions?

adequate

all aspects of management are clearly discussed; budgets and time allotted for each task are realistic

Team Composition:

Does the lead principal investigator have successful management history and experience leading collaborative teams? Is it clear that all key personnel are committed to making significant contributions to the project? Do team members have complementary skills?

above average

the team seems experienced and all are making significant contributions

Communication Of Results:

Is there a clear plan for comprehensive and cohesive reporting of project progress to the CALFED community?

above average

will reach a wide audience

Additional Comments:

I was confused with the title--I was expecting to read that ecological field studies were to be conducted in simultaneously with the hydrological studies

Collaboration Panel (Discussion) Review

The main detractor for the primary was that the two institutions did not seem balanced, and did not seem to have

Collaboration Panel Review

adequate collaboration at the organizational level, despite the coordination among researchers. For example, EBMUD staff is assisting only with field work and may be conducting the ecological observations only after the hydrological portion of the project is completed. On the other hand, the team composition at LBNL is adequate according to standards of conducting a hydrologic study.

In contrast, secondary reviewer gave it a superior rating for collaboration. Description of collaboration was well described between researchers involved. Pg 16 "the 3 primary researchers will develop... conceptual models." Work between teams was explicitly stated, but it was hard to identify leaders of subprojects. However, secondary rated it inadequate for communication and reporting of results (just progress reports) and project management.

Technical Synthesis Panel Review

Proposal Title

#0056: Water Partitioning and Availability: Impact of Hydrology Shifts on Ecosystem Processes in a Bay-Delta Watershed

| Final Panel Rating |
|--------------------|
| adequate |

Technical Synthesis Panel (Primary) Review

TSP Primary Reviewer's Evaluation Summary And Rating:

Dr. Rohit has submitted a big-budget (1.4 M) NSF-type proposal to conduct basic research on the fluxes of water in a small (2617-acre) watershed. The major benefit if the proposal is that it will provide new information on the partitioning of water that will be useful for modeling streamflow in California. The weakness of the proposed work is three fold: (1) the proposed research is not of direct benefit to decision makers; (2) results for the test basin are unlikely to be transferable throughout the California Bay-Delta system; and (3) the proposed research is not focused on ecological processes. The proposed research may fit better in a basic science program (e.g., NSF) than in CALFED's applied research program. The main comments from the external reviewers include (1) "Not only is the idea important, it is long overdue. So much in hydrology is now taken for granted in California, even though many of the conceptual and numerical models were developed for California. (2) the proposal is poorly matched with the program solicitation; (3) new methods developed to monitor soil moisture does not fit with the rest of the proposal; (4) the applicants should include considerations of fog drip

Additional Comments:

Dr. Rohit has submitted a big-budget (1.4 M) NSF-type proposal to conduct basic research on the fluxes of water in a small (2617-acre) watershed. The major benefit if the proposal is that it will provide new information on the partitioning of water that will be useful for modeling streamflow in California. The weakness of the proposed work is three fold: (1) the proposed research is not of direct benefit to decision makers; (2) results for the test basin are unlikely to be transferable throughout the California Bay-Delta system; and (3) the proposed research is not focused on ecological processes. The proposed research may fit better in a basic science program (e.g., NSF) than in CALFED's applied research program. The main comments from the external reviewers include (1) "Not only is the idea important, it is long overdue. So much in hydrology is now taken for granted in California, even though many of the conceptual and numerical models were developed for California. (2) the proposal is poorly matched with the program solicitation; (3) new methods developed to monitor soil moisture does not fit with the rest of the proposal; (4) the applicants should include considerations of fog drip

Technical Synthesis Panel (Discussion) Review

TSP Observations, Findings And Recommendations:

Although one reviewer applauded the science in the proposal, the panel did not think that the research proposed in this project will be of direct benefit to decision makers. This proposal may fit better in a basic science program (e.g. NSF). It is poorly matched with the program solicitation. There are no major science flaws in the proposal however. The benefit of the proposal is that it will improve understanding and modeling of hydrologic processes in California. It is a good proposal and project, but may not be the best focus right now. The science is above average but the budget is excessive and there are uncertainties about the relevance of this proposal.

Technical Synthesis Panel Review

Ranking is adequate.

Technical Review #1

proposal title: Water Partitioning and Availability: Impact of Hydrology Shifts on Ecosystem Processes in a Bay–Delta Watershed

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

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| Comments | The opening paragraph in Project Purpose (section 1) reiterates the priorities identified in the solicitation. However, the goals and objectives (section 1.1) addressed by the proposed research do not address these priorities. In particular, there is no ecological component to the proposed research. |
| Rating | poor |

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full–scale implementation project justified?

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| Comments | The proposed research is directed at providing detailed knowledge of the water budget in a semi-arid area of the CALFED watershed. This work will not develop the knowledge needed to address water management challenges with respect to maintaining and restoring wetlands, estuaries and associated endangered species that are identified as a priority by the solicitation. Contrary to claims of the proponents that this research will “further our understanding of the interaction between hydrology and ecosystem processes,” it will not. |
| Rating | |

Technical Review #1

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| | poor |
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Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

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| Comments | The plan of work identifies three concurrent tasks. None of the three tasks will examine directly the influence of hydrologic processes on any aspect of the CALFED ecosystem, excepting the physical processes operating within the boundaries of the study site. |
| Rating | poor |

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

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| Comments | One of the principal investigators has carried out a preliminary investigation at the site of the proposed research. The activities described in the work plan represent a natural extension of this previous work, albeit severely limited within the scope established by the solicitation. |
| Rating | very good |

Monitoring

If applicable, is monitoring appropriately designed (pre–post comparisons; treatment–control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

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| Comments | |
| Rating | |

Technical Review #1

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| | not applicable |
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Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

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| Comments | The proponents plan to disseminate their results through the standard means within the scientific community and through programs reaching the schools by their host institutions. The proposal does not address how they will effectively communicate results of their work into or within the CALFED program. |
| Rating | fair |

Additional Comments

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| Comments |
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Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

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| Comments | The principal investigators (Salve and Doughty) exhibit solid records of past work in the area of study encompassed by the work plan. |
| Rating | very good |

Budget

Is the budget reasonable and adequate for the work proposed?

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| Comments | not reviewed |
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Technical Review #1

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| Rating | not applicable |
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Overall

Provide a brief explanation of your summary rating.

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| Comments | <p>This proposal appears to be badly matched with the solicitation. The proposed research will investigate in detail the soil moisture dynamics within a single, small, semi-arid watershed that drains into the reservoir of the San Pablo dam. Numerical models will be used to integrate results of laboratory measurements and of monitoring conditions in the field. Despite the breadth promised in the title of this proposal, the research will not address links between hydrologic variation and ecosystem processes. There is no ecological component to the proposed field investigations (section 3.2.1), the modeling (section 3.2.2), or the development of new methods (section 3.2.3). Without comparative analysis across a number of similarly sized, but hydrologically diverse watersheds, it will not be possible for the proponents to address effects on aquatic ecology endpoints (identified in the solicitation) with the type of information that this research will develop.</p> |
| Rating | fair |

Technical Review #2

proposal title: Water Partitioning and Availability: Impact of Hydrology Shifts on Ecosystem Processes in a Bay-Delta Watershed

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

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| Comments | The goal of this project - to develop a quantitative understanding of the hydrological cycle within a watershed, to facilitate management plans for water allocation - is timely and important, and fits nicely into the topics of particular interest for the CALFED 2004 Science Program. In addition, ecohydrology is an emerging discipline within ecology and hydrology, so this is of interest to the broader scientific community. Within the proposal, the scientific objectives and hypothesis are clearly stated and are consistent throughout the proposal. The hypothesis "The amount of water partitioned into each component of the hydrological cycle...varies seasonally and annually" is simple and straight-forward, and the study is designed to directly test it. |
| Rating | excellent |

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full-scale implementation project justified?

Technical Review #2

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| Comments | The background section provides a good overview of the current literature in hydrology and water-shed modeling. The justification described in section 4 meshes nicely with this literature review, and clearly describes how this project will advance ecohydrological knowledge in general, and in particular how this will be of benefit to CALFED. Conceptual models demonstrating seasonal partitioning of water and annual variability are provided (Fig. 2). However, I found them to be almost too symplistic, given the complexity of the system described in the proposal. |
| Rating | very good |

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

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| Comments | The approach appears to be well-designed and will build on prior research by one member of the research team in a nearby, similar watershed. The research has been divided into 3 concurrent tasks: 1) monitoring the hydrological cycle; 2) developing a numerical model; and 3) developing and testing new methods to measure soil moisture content. Monitoring stations appear to be well distributed in the upper part of the watershed, along the watershed slopes and near the main pond. However, Fig. 8 does not show any monitoring stations in the southern part of the watershed (in the riparean zone), although the text (Sect. 3.2.1.2) indicates that discharge will be monitored in the riparean zone. This discrepancy disturbed me a bit. The numerical modeling will draw on expertise in the ESD at the Berkeley Lab. This model is reliant on the data collected from the field monitoring. As the monitoring and modeling will be done concurrently, there is the flexibility to change |
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Technical Review #2

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| | the monitoring to better suit the modeling needs, which is good. The development of new methods section is not well-integrated into the monitoring and modeling components. The approach appears to be feasible and should add to the knowledge base about ecohydrology in general and water partitioning in California in particular, which should provide useful information to decision makers. |
| Rating | very good |

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success?
Is the scale of the project consistent with the objectives and within the grasp of authors?

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| Comments | The approach is fully documented, although there appear to be some discrepancies between Fig. 8 and the text about the location of monitoring stations. And the development of new methods is not well-integrated into the monitoring and modeling sections, to show where these new methods will replace conventional methods in monitoring, or if the modeling component is dependent on results from these new methods. The scale of the project is consistent with the project objectives and is within the expertise of the assembled research team. |
| Rating | very good |

Monitoring

If applicable, is monitoring appropriately designed (pre-post comparisons; treatment-control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

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| Comments | Monitoring is a significant component of the project, but there are no treatments and thus no need for pre-post or treatment comparisons. The results of the |
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Technical Review #2

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| | monitoring will be used for numerical modeling. The results from this watershed will be applicable to other watersheds in the Bay-Delta region, as will be the procedure for model development. |
| Rating | very good |

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

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| Comments | This proposal will test new methods to measure soil moisture that have been developed at the ESD at the Berkeley Labs, and hopes to adapt these technologies for the proposed research. However, it does not appear that any new products will be developed during this research, just testing of existing products. The models to be developed will be relevant to other watersheds and thus could contribute to a larger data management system. However, the means by which this will be accomplished are not explicitly stated in the proposal. Interpretable outcomes with respect to water partitioning in Bay-Delta watersheds will result. |
| Rating | very good |

Additional Comments

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| Comments |
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Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

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| Comments | The investigators appear to be highly qualified for this research. Drs. Salve and Doughty are scientists with considerable experience in hydrological |
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Technical Review #2

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| | monitoring and modeling, in similar watersheds to that which will be used for this project. Mr Tripp, who will oversee the management of the experimental site and monitor ecological processes, has the appropriate scientific training and job experience. The infrastructure and support necessary to accomplish this project are available. |
| Rating | excellent |

Budget

Is the budget reasonable and adequate for the work proposed?

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| Comments | The tasks to be completed have been carefully thought out, and thus the hours and money budgeted appear to be appropriate for each. The proximity of the study site to the Berkeley Labs helps to keep travel costs down. Overall, the budget seems reasonable for this project. |
| Rating | very good |

Overall

Provide a brief explanation of your summary rating.

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| Comments | This is an interesting, timely project that should produce scientifically meaningful results with a broader application. The assembled research team has th expertise necessary to effectively implement the project, and the budget seems reasonable. This project should provide data on water partitioning and ecohydrology in a watershed in the Bay-Delta region. This data will be very useful to CALFED for water budgeting purposes, as will the models to be developed. |
| Rating | very good |

Technical Review #3

proposal title: Water Partitioning and Availability: Impact of Hydrology Shifts on Ecosystem Processes in a Bay-Delta Watershed

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

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| Comments | <p>ARE THE GOALS, OBJECTIVES AND HYPOTHESES CLEARLY STATED AND INTERNALLY CONSISTENT? Yes. The applicants clearly and consistently state their objectives and refer back to them throughout the proposal. I have to say, their hypothesis, "we hypothesize that the amount of water partitioned into each component of the hydrologic cycle, and therefore available to various ecosystem processes, varies both seasonally and annually in watersheds of the BayDelta." although testable within their specific watershed, is pretty much a statement of the obvious. The hypothesis does little to demonstrate the true value and depth in the research the applicants are proposing. In reading through the application, the applicants stick to this hypothesis as a theme without overemphasizing it. I think that CALFED unnecessarily over-emphasized the requirement of hypotheses in this round of proposals. The applicants appropriately responded by half-heartedly conforming to this requirement.</p> <p>IS THE IDEA TIMELY AND IMPORTANT? Not only is the idea timely, it is long over due. So much in hydrology is now taken for granted as applicable to California, even though many of the conceptual and numerical models were developed for temperate climates. The watersheds of the Bay-Delta system are primarily semi-arid and little has been done to explore the implications of applying models from temperate systems</p> |
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Technical Review #3

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| | in this setting. It is refreshing to see a proposal really testing the basic assumptions about hydrologic partitioning behind so many of the rainfall runoff models in use and so critical to ecologic functioning. |
| Rating | excellent |

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full-scale implementation project justified?

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| Comments | <p>IS THE STUDY JUSTIFIED RELATIVE TO EXISTING KNOWLEDGE? Yes. The applicants effectively highlight a gap of existing knowledge about the importance of vadose zone hydrology in seasonal water partitioning. Furthermore, the relate this to the topical management concerns of water allocation and hydrologic requirements of watershed flora.</p> <p>IS A CONCEPTUAL MODEL CLEARLY STATED IN THE PROPOSAL AND DOES IT EXPLAIN THE UNDERLYING BASIS FOR THE PROPOSED WORK? Of the three science program and two ecosystem restoration program reviews I conducted, this application presented the most effective conceptual model. The applicants successfully conveyed how their conceptual model (Figure 2) formed a basis for their study and assumptions, and how their approach would test these assumptions. As with all conceptual models, they are simplification of reality. My only concern with this conceptual model is the lack of consideration of coastal fog in the summer months (see more detailed comments in Additional Comments section).</p> <p>IS THE SELECTION OF RESEARCH, PILOT OR DEMONSTRATION PROJECT, OR A FULL-SCALE IMPLEMENTATION PROJECT JUSTIFIED? The applicants limit their scope to a small individual study site. Given the relative lack of study on water partitioning and vadose zone hydrology,</p> |
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Technical Review #3

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| | this seems an appropriate level to start at. |
| Rating | excellent |

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

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| Comments | <p>IS THE APPROACH WELL DESIGNED AND APPROPRIATE FOR MEETING THE OBJECTIVES OF THE PROJECT? The approach is well thought out and builds off of an extensive record of previous research by the applicants. I have no overall concerns with the approach. I do have some minor specific questions about some of the details of the implementation. I have listed these below.</p> <p>1. With respect to the development of 1D, 2D and 3D models in task 2, working up from 1D to 2D to 3D representations seems a logical stepwise framework. It is unclear to me a) whether this is necessary or b) what the authors intend to do with the information from the 1D and 2D models. The underlying premise seems to be that the 1D and 2D models "will not be adequate to fully capture watershed-scale behavior" and that the 3D model is the ultimate goal. This seems a reasonable assumption, but if this is a case, why not just start with the 3D model? Usually, for in channel flow models at least, the value in an inter-comparison of 1D, 2D and 3D models is in highlighting the extent to which simpler lower-dimension models can adequately represent the processes of interest. There seems to be a trend of assuming the 3D models are absolutely necessary, but it really depends on what specific processes you are interested in. I recognize that vadose zone and groundwater models always maintain the vertical component when decreasing dimensions, whereas surface water models always maintain the longitudinal</p> |
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Technical Review #3

(streamwise) component. Thus, there may be some conceptual differences. However, I am not sure the effort in developing 1D and 2D approaches is necessary unless some sort of detailed inter-comparison is a specific goal.

2. Within task 3, I take the applicants point about the advantages of GPR over more standard techniques of characterizing water contents and physical structure in the subsurface because it is quick to use and a non-intrusive in-situ measurement. However, it is not apparent from this application how the applicants will deal with many of the problems in the data collection of GPR that more standard techniques avoid. GPR would provide an interesting direct comparison of a 2D transect with the 2D model or a cross section of the 3D model. However, the GPR is only a snap shot in time, whereas those point-scale measuring devices give actual time series of data to compare with model dynamics. It is also unclear to me how the GPR tracks its position and how post-processing of the signal rectifies positional errors (e.g. along a slope, along a non-straight path). Simply identifying two end pins of a GPR transect may be inadequate, whereas total-station or RTK-GPS tracking of a transect will add some additional cost. I do question the validity of trying to use a GPR to add to a suite of existing tools, but I am sceptical that it will provide the 'silver-bullet' the applicants might be looking for. This seems analogous to the excitement over ADVP and ultrasonic Doppler in surface water hydrology. All approaches have their strengths and weaknesses.

IS THE APPROACH FEASIBLE? Everything the applicants are proposing seems feasible.

ARE RESULTS LIKELY TO ADD TO THE BASE OF KNOWLEDGE? Both within the CALFED context and wider scientific context, this project would represent a significant contribution to the existing knowledge base.

Technical Review #3

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| | <p>IS THE PROJECT LIKELY TO GENERATE NOVEL INFORMATION, METHODOLOGY, OR APPROACHES? This project aims to contribute novel information and approaches on three fronts: conceptual understanding, numerical modelling and field data collection. I would be surprised if the outcomes of the project were not considered original contributions worthy of publication.</p> <p>WILL THE INFORMATION ULTIMATELY BE USEFUL TO DECISION MAKERS? I think this is where the applicants face the biggest challenge. There is no doubt that ultimately this information could be useful to decision makers. This will depend on how the applicants are able to relate their findings to relevant issues. They have already highlighted how they might do this. However, because this is at one level an interesting piece of basic research, it may take time beyond the lifetime of this project to ultimately provide information that is useful to decision makers at the Bay-Delta system wide scale. The link to restoration is not immediately obvious, although the implications on restoration are not difficult to envision.</p> |
| Rating | very good |

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success?
Is the scale of the project consistent with the objectives and within the grasp of authors?

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| Comments | <p>IS THE APPROACH FULLY DOCUMENTED AND TECHNICALLY FEASIBLE? The approach is very thoroughly documented and relies on a clever application and tweaking of existing model codes and field techniques. It is therefore technically feasible.</p> <p>WHAT IS THE LIKELIHOOD OF SUCCESS? High.</p> <p>IS THE SCALE OF THE PROJECT CONSISTENT WITH THE OBJECTIVES AND WITHIN THE GRASP OF AUTHORS? Yes.</p> |
| Rating | |

Technical Review #3

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| | very good |
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Monitoring

If applicable, is monitoring appropriately designed (pre–post comparisons; treatment–control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

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| Comments | IF APPLICABLE, IS MONITORING APPROPRIATELY DESIGNED (PRE-POST COMPARISONS; TREATMENT-CONTROL COMPARISONS)? This project has a strong field data collection component, but it is not a 'restoration' monitoring program as I suspect is what is meant by monitoring. My only comments on the field data collection are provided above in the approach section. ARE THERE PLANS TO INTERPRET MONITORING DATA OR OTHERWISE DEVELOP INFORMATION? Field data will be an instrumental part of testing the conceptual models and developing the numerical models. |
| Rating | not applicable |

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

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| Comments | ARE PRODUCTS OF VALUE LIKELY FROM THE PROJECT? The three objectives of this project are to produce field data, working numerical models and new field techniques; each of which is valuable product in and of itself. Arguably, any one of these products could justify the project. However, the combination of the three makes this a very strong project. ARE CONTRIBUTIONS TO LARGER DATA MANAGEMENT SYSTEMS RELEVANT AND CONSIDERED? Not particularly. Contributions to larger data management systems are not entirely relevant to this application. There are |
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Technical Review #3

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| | <p>adequate provisions for how they will manage the data internally, but the applicants provide little indication of whether this data (not just results and interpretation) will be disseminated publicly. Given the site-specific nature of the data, I'm not sure that the raw data or its management will be of major value to the wider community.</p> <p>ARE INTERPRETIVE (OR INTERPRETABLE) OUTCOMES LIKELY FROM THE PROJECT? To the extent that the conceptual and numerical model outputs provide additional insight into hydrologic partitioning in the vadose zone and its impacts on ecologic functions, there is a wealth of interpretative data that should come out of this project. I suspect that their interpretations of their findings are likely to have more impact within the CALFED community than the modelling and field methodology products (which are of greater interest to the broader scientific community).</p> |
| Rating | very good |

Additional Comments

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| Comments | <p>On page four of the proposal the applicants state: "For this research effort, we consider rainfall, which occurs during the winter (~October-April), to be the predominant source of recharge in the water-stressed watersheds of the Bay-Delta system. We further assume that in these watersheds, water budgets are very different from wet settings, with the temporal variability in recharge, subsurface storage in the extensive vadose zone, and ET dominating the allotment of water to components of the hydrologic cycle." Given the location of the study site in the semi-arid east bay hills, these appear to be perfectly reasonable assumptions (and I doubt most reviewers would challenge this assumption). However, I would strongly encourage the applicants to at least conceptually more closely consider the role that fog-drip might play given the predeominance of coastal fog in the summer</p> |
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Technical Review #3

months in this area. I would not dispute that rainfall is the predominant source of recharge in the study watershed. However, as hydrologists I think we have especially neglected the potential roll fog plays as a major hydrologic input at a time of year (summer) when the system is most water-stressed and water availability is absolutely critical to the hydroecology. Dawson (1998), who has mainly focused on the hydrologic and ecological importance of fog in coastal redwood forests, reported that even in open grasslands (similar to vegetation at applicant's study site) annual hydrologic input from fog drip can account for 17% of total water inputs. Burgess and Dawson (2004) revisit the ecological significance of this in more detail, but both studies give some insight into some relatively simple and cheap monitoring techniques to measure fog drip in the field. When considering the ecohydrology significance of your study, this could add considerable insight. I am not aware of too much research that has been done in this area, and I imagine it has more or less been entirely neglected in vadose zone hydrology. Worth thinking about...

On page fourteen of the proposal the applicants state: "Note that with modeling recognized to be a part of the project from the outset, additional site characterization data will be collected as the model is being developed, greatly decreasing the uncertainty of the model." I would encourage the applicants to be very careful with these sorts of statements, and much more specific about what is meant by uncertainty. As stated, the authors imply that they can reduce structural uncertainty in the model through more data collection. This is not necessarily the case. More field data is often incorrectly assumed by modellers to reduce uncertainty. It might be used in specific cases to reduce parametric uncertainty in a specific model realization. However, it is not necessarily likely to reduce the structural uncertainty in the model's ability to represent a particular process at a

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| | particular location or in general. For an excellent review of uncertainties in environmental models, see (Van Asselt and Rotmans, 2002). |
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Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

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| Comments | WHAT IS THE TRACK RECORD OF AUTHORS IN TERMS OF PAST PERFORMANCE? The track record of the authors is excellent and demonstrates both their capability to carry out the proposed project and the likelihood of dissemination in well respected peer-review journals. IS THE PROJECT TEAM QUALIFIED TO EFFICIENTLY AND EFFECTIVELY IMPLEMENT THE PROPOSED PROJECT? The project team is perfectly qualified to deliver some very original scientific research in an efficient manner. DO THEY HAVE AVAILABLE THE INFRASTRUCTURE AND OTHER ASPECTS OF SUPPORT NECESSARY TO ACCOMPLISH THE PROJECT? The applicants come from two organizations with extensive equipment supplies, computational infrastructure and resources to support such a project. More specifically, the applicants have used both the specific models and the field equipment they propose to modify extensively in the past. |
| Rating | excellent |

Budget

Is the budget reasonable and adequate for the work proposed?

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| Comments | At \$1.4 million, one does wonder how beneficial such a focused project on a small little catchment upstream of a reservoir in the East Bay hills will be to the overall CALFED |
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Technical Review #3

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| | program. However, the applicants convincingly argue that their research is addressing a basic question of hydrologic partitioning that is likely to have system-wide significance. Given the potential scientific significance of the work proposed within this proposal, the budget does seem reasonable. The details provided in the budget seem to suggest that it is adequate as well. |
| Rating | good |

Overall

Provide a brief explanation of your summary rating.

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| Comments | <p>This was a very thoughtfully crafted proposal building off the applicants' previous research into an understudied area with respect to restoration. Unfortunately, this reviewer's expertise is not specifically in vadose zone hydrology, which is at the core of this proposal. However, thanks to the proposal's thorough yet concise background and introduction, this proposal was easy to read and review. My only minor criticism of the review as a whole was that the amount of space dedicated to background and reporting previous work was excessive when compared to the proposed work. It did however demonstrate a clear grasp of the problems they are addressing.</p> <p>I have provided a list of the few references I cited in this review below:</p> <p>References:</p> <p>Burgess SSO and Dawson TE. 2004. The contribution of fog to the water relations of <i>Sequoia sempervirens</i> (D. Don): foliar uptake and prevention of dehydration. <i>Plant Cell and Environment</i>. 27: 1023-1034.</p> |
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Technical Review #3

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| | <p>Dawson TE. 1998. Fog in the California redwood forest: ecosystem inputs and use by plants. <i>Oecologia</i>. 117(4): 476-485.</p> <p>Van Asselt MBA and Rotmans J. 2002. Uncertainty in integrated assessment modelling - From positivism to pluralism. <i>Climatic Change</i>. 54(1-2): 75-105.</p> |
| Rating | excellent |